

Notes: doc John More. ①

B209

Bryce Springs. FRIDAY; 12/24/76

Harry last saw John & Betty More in 1972 (\leftrightarrow Anza Bryce) we called them Monday evening to see if they would join us here for a day. And sure enough they did, driving from Riveirede to meet us at 1000. His car was a bit higher sprung than our rented Cutlass; we nevertheless left it about $\frac{1}{2}$ way up Fish Creek and walked mostly in the wash approaching Sandstone Canyon.

Johnny was very positive about historical mining and about doing this with Harry — whom he recalled as a graduate student!

Some points about the mineral: Francis entered a highly competitive, cut-throat arena in the department. He was a bright young embryologist when he took his fellowship at Stanford [? how different from selection process for the fellowship ^(@ over) {some minor physical disability may have sufficed?}] and it was already settled that he would return as an instructor to teach Zos 1-2 (as it happened the V-12 pre-meds); but meantime he switched to Neurospora. He had also (mildly) antagonized Professor Baeth by switching at the last minute to Pollister as his dissertation sponsor. Real reason not recorded: he told John it was because "Pollister had never had a good student!" (He + John were fellow grad. students; the temperature-electrode expt. denied John some of John's observations; injection of the developmental index may be spurious: gastrulation can be delayed while other maturation processes continue.)

When Francis returned to Columbia, the great lights (Orens, Dobzhansky, even Baeth) were not too thrilled about his furnishing biochemical — perhaps not even about his invading genetics. [This, not the choice of organism, was the problem. Later when John, plus myself Francis recommended that I be somewhat cautious about coming back to Columbia, Dobzhansky still thought microbial genetics was a "flea in the soup".]

Consider implications of this attitude as a danger for Avery! \Rightarrow

{Paradox that it was Dobry who brought Griffith to the attention of most geneticists via his comments in GAToss. 3.]

Francis was viewed as an energetic, aggressive investigator — perhaps almost too much so in competing for space etc. (Briskunual work needs more.) — he was just in the wrong field. Research was the paradigm for promotion; but it needed to be sparked by an offer elsewhere; and jobs were scarce. When Jhuny was offered a tenure position at Hopkins he was able to negotiate a promotion for himself but only on the further condition (unbreakable!) that Francis also be elevated in a reasonable time.

Francis set up his own senior series in the department. (Doubtless the one I also talked about in Fall '45) — which had many, since-illustrious visitors. "D + D never came," to Francis great hurt!

Betty describes this as prima-donna complex. When Jhun became chairman he spent almost his full time trying to ameliorate the personal feuds and why the department members get along with one another as decent human beings.*

As to graduate students, it took Jim a while to focus on 1945-8; he thought later there were quite a few. During the war, graduate students were scarce; perhaps of lower quality. They would tend to be prompted by the senior faculty; and in genetics in particular, D + D were better known, and had earlier access to them in the standard course sequence. It would take something for Francis' reputation to be established, to be an independent

* At Stanford only Pharmacology comes close to this level of internal inconsistency. An important subject for sociological inquiry: how much academic energy is wasted in such conflicts? What are the preconditions for effective cooperation? Is recognizability; leadership an adequate answer?

attractant. { all of which facilitated a niche for myself! } . (3)

I tried to press John on the question of how that atmosphere might have shaped Francis' own research program; but did not get a clear focus on the affective-cognitive intersections. He just returned to the theme it was "difficult."

[I should have turned the question to John's own career, where he might be better informed! But he would probably have been too modest and self-deprecating to come through. But John is also plainly much more diplomatic (alb. circumspect) than Francis].

He focussed on Selig Hecht as a victim: "Brilliant, Jewish, aggressive in research, ^{fresh} resounding at gatherings, autonomous but isolated at Pyskin Hall.— felt very lonely. John tried to mediate — e.g. to persuade Schaefer, with some difficulty, to hold faculty mtg. once at Pyskin. — "brought tears to Hecht's eyes!" "Entertaining them royally & cordially, too... } when they did come over!

P.S. Francis continued to teach 300101-102 because that was what he was originally hired for; who else could have done it!; he was too loyal to the department.

{ The nabobs' ~~attitudes~~ attitudes may also have been conditioned by his being home-grown. Wouldn't they inevitably have difficulty effacing their recollections of him as a green student from their overall appreciation of him. Is this a verifiable possibility, ~~that~~ that decision is best accepted in such circumstances. }

Note: a dozen cartons of Dept. records were deposited in Edemaria by J. Moore 14 August 1962. In addition about 6 on the history of genetics were sent to the Am. Phil. Soc. L.C. Darden's papers are also there! He does not know what was discarded. No handwritten notes

Cy Levinton knows that many E.B. Wilson papers were in the "Wilson house" he used to rent at Woods Hole. (These could be priceless for history of American Biology < 1940).

Sorry

12/24/76.

"Ryan evaluated [Joh] J with starting his
work as reverse-mutator in bacteria".

(I may have discussed research programs with
him as backup to recombination &
probably did look at meth⁻ → meth⁺
(of Robin + Harris); possibly an eye
to meth⁺ → meth⁻.